

Canadian Association of General Surgeons Evidence Based Reviews in Surgery. 12.

Primary repair for penetrating colon injuries

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Selected article

Nelson R, Singer M. Primary repair for penetrating colon injuries [Cochrane review]. In: *The Cochrane Library*; Issue 3, 2003. Oxford: Update Software.

CAGS Evidence Based Reviews in Surgery

In September 2000, the Canadian Association of General Surgeons (CAGS) initiated a program titled CAGS Evidence Based Reviews in Surgery (CAGS-EBRS) to help practising clinicians improve their critical appraisal skills. During the academic year, 8 clinical articles are chosen for review and discussion. Both methodologic and clinical reviews of the article are made by experts in the relevant areas. The *Canadian Journal of Surgery* publishes 4 of these reviews per year. Each includes an abstract of the selected article and summarizes the methodologic and clinical reviews. We hope that readers will find these useful and learn skills that can be used to evaluate other articles. For more information about CAGS-EBRS or about participating in the program, email mmckenzie@mtsina.on.ca

Abstract

Objective: To determine if the treatment of penetrating colonic injury must include fecal diversion at or proximal to the injury, to avoid sepsis and mortality. **Data source:** Studies were identified by searching MEDLINE 1966–2001, the Cochrane Controlled Trials Registry and EMBASE. **Study selection:** Studies were included if they were randomized controlled trials comparing outcomes of primary repair versus fecal diversion in the management of penetrating colon injuries; 5 studies were identified. **Outcome measures:** Operative mortality, total complications, total infectious complications, intra-abdominal infections, abdominal infections excluding dehiscence, and wound complications including and excluding dehiscence. Penetrating abdominal trauma index (PATI) and length of stay were included when available. **Results:** PATI did not significantly differ between groups; neither did mortality (odds ratio [OR] 1.7, 95% confidence interval [CI] 0.51–5.66). However, total complications (OR 0.28, CI 0.18–0.42), total infectious complications (OR 0.41, CI 0.27–0.63), intra-abdominal infections (OR 0.59, CI 0.38–0.94), abdominal infections excluding dehiscence (OR 0.52, CI 0.31–

0.86) and wound complications including (OR 0.55, CI 0.34–0.89) and excluding dehiscence (OR 0.43, CI 0.25–0.76) all significantly favoured primary repair. **Conclusions:** Primary repair of penetrating colon injuries is as safe as fecal diversion and has a lower complication rate.

Commentary

The issue of primary repair for penetrating colon injuries is pertinent at this time. Whether to proceed with primary repair, including possible resection and anastomosis, or to perform the tried-and-true stoma is a question definitely on the minds of trauma surgeons today.

Nelson and Singer, the authors of this Cochrane review,¹ sought to determine whether appropriate treatment of penetrating colonic injuries must include fecal diversion with an intestinal stoma at or near the site of injury. Extensive searches were made of 3 major medical databases for prospective randomized controlled trials of patients with penetrating colon injuries that compared primary repair sans stoma against fecal diversion with a stoma at or proximal to the point of injury. Patients with rectal injuries were excluded. All study patients must have had laparotomies that confirmed penetrating injury of

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the colon. Type of repair had to have been decided by randomization after confirmation of the colon injury. Patients in one comparison group must have undergone repair of the injury with primary closure or resection with anastomosis; those in the other group, one of: exteriorization of the injury as a stoma, resection of the injured segment with an end stoma, or repair of the injury with proximal fecal diversion. Outcomes under consideration were as stated in the Abstract.

The online search for studies was not limited to articles in English. The terms used in the database searches were *colon trauma*, *colostomy*, *primary repair*, *prospective* and *randomized*. References cited in the articles identified were also searched; nonrandomized studies were reviewed; and the directors of 2 trauma units were asked if they were aware of any unpublished studies. As stated, 5 studies were identified that met the criteria.

This process was carried out according to established Cochrane guidelines and is unlikely to have missed important published studies in the area. It may have been desirable to contact more than 2 trauma experts, and also to ask colorectal surgeons; but this is a minor point, since important work done in the area would probably be well known.

Quality was assessed with use of the *Cochrane Reviewers Handbook*. After assessing the methodological quality of the 5 studies independently, reviewers settled discrepancies by discussion. Adequacy of concealment of patient allocation was categorized by predetermined standard criteria: *A* if satisfactory, *B* if unclear, and *C* if unsatisfactory. The Cochrane Collaboration now assesses quality on 4 points only: blinding of allocation (mandatory), blinding of intervention, outcome, and completeness of follow-up. The fact that at least 2 reviewers analyzed each paper reduces the likelihood of errors, both random and systematic, and the assessments performed appear to be reproducible.

Nelson and Singer do not seem to have done any other formal analysis of study validity. It would have been desirable had they used one of the several instruments available that assess randomized trials by their overall quality.

All 5 of the trials identified were included; methodological problems were ascribed to each. Stone and Fabian² excluded 48% of their prospective study patients, which makes questionable whether their findings can be generalized. Their allocation method was unacceptable, as well. The study by Chappius and associates³ was more inclusive, but their allocation method was unclear. In the study led by Falcone,⁴ patient allocation was satisfactory and very severely injured patients were included, but an intracolonic bypass device was used. Sasaki and colleagues⁵ applied no exclusion criteria, but used a poor method of allocation. Patient allocation was satisfactory in the study by Gonzalez and coauthors⁶ and no exclusion criteria were applied, yet their analysis excluded 5 patients who died in the early postoperative period. (Appropriately, Nelson and Singer adjusted the mortality in this trial to include those 5 early deaths.)

One concern with the studies is that amounts of abdominal fecal soilage present were not clearly stated. In 1 study, patients with any notable amount of soilage were excluded. This was not stated in the other studies, which were assumed to include all such injured patients, regardless of contamination. Clear indication of how much gross contamination occurred would have been desirable. Based on this analysis and in the absence of this information, surgeons may be less inclined to proceed with a primary repair in a setting of gross contamination.

Four of the studies reported PATI scores. The mean was 28.9 for the primary-repair patients and 25.8 for the stoma patients, suggesting that primary-repair patients had more serious injuries than those in the diverted groups.

Nelson and Singer tested all possible outcomes for heterogeneity to determine the likelihood that the differences observed were due to something other than chance. It is highly desirable for the results of these tests to be non-significant. When statistically significant heterogeneity between studies exists, differences in patients, exposures, outcomes or study design may be responsible for the varying treatment effect. In this situation, combining results is generally inappropriate. (Even when test results for heterogeneity are not significant, caution is still necessary when combining studies.)

For the 5 studies included in the meta-analysis, the χ^2 test for heterogeneity for the outcome "overall mortality" was non-significant ($p = 0.77$). For the outcomes "intra-abdominal infection" and "wound complication," there was likewise no significant heterogeneity. But for the outcomes "total complications" and "infectious complications," test results for heterogeneity were significant. In both cases, 1 trial accounted for the heterogeneity; when it was excluded, test results for both outcomes became non-significant. In summary, the results of the 5 studies can be combined with some assurance that differences are due to chance alone for the outcomes "overall mortality," "intra-abdominal infection" and "wound complication."

One of the most important aspects of meta-analysis is that it ensures that studies of large numbers of patients are given more weight than studies of small numbers. The overall results can be thought of as a weighted average of the results of the individual trials. Clinically important differences in outcome that are not statistically significant in small trials may become significant when combined.

In the meta-analysis under discussion, the difference in overall mortality between the 2 groups was non-significant: 2.9% for the primary-repair group, compared with 1.8% for the diverted group (OR 1.70, CI

0.51–5.7; since the interval contains 1, the difference is not statistically significant). The rates of total complications and infectious complications were significantly lower in the primary-repair group; however, as previously noted, the test results for heterogeneity were significant, which makes this difference difficult to interpret. Although the result became non-significant by excluding 1 trial, no reason is apparent to exclude that trial. The most reasonable conclusion from this analysis may be that the rate of total complications was not higher in primary-repair patients.

The rate of intra-abdominal infection was significantly lower in the primary-repair group (OR 0.59, CI 0.38–0.94), for whom wound complications were also less common (OR 0.55, CI 0.34–0.89). Nelson and Singer calculated these values with and without wound dehiscence; their rationale for excluding this complication is unclear. They stated that dehiscence could be a result of technical error, but that is true of virtually every aspect of surgery. However, the fact that the difference was significant whether or not wound dehiscence was included actually strengthens the argument that primary closure was superior.

Nelson and Singer were unable to calculate odds ratios for length of hospital stay due to lack of data, but from what was available they calculated a mean length of stay of 12.7 days for the primary-repair group and 16.1 days for the diverted patients. (These numbers did not include hospitalizations of diverted patients for closure of their colostomy, although it is probable that most eventually underwent stoma closure.) It would not be unreasonable to conclude that primary closure does not appear to increase the length of hospital stay.

The only significant differences in outcome shown by this meta-analysis were that patients who underwent primary repair had lower rates of intra-abdominal infections and wound complications. In both sets of results, the 95% confidence intervals were narrow, indicating considerable precision.

In general, the outcomes considered in this overview are those surgeons would use to judge the relative merits of the treatment options. With mortality being the same and complications being lower, most surgeons would think that the most important outcomes had been considered. It would have been desirable to have complete length-of-stay data, although when closure of the stoma is included, the diverted group likely would have had longer hospital stays.

It is apparent that both treatment options are safe and acceptable. When this is the case, the most desirable measure of which treatment is superior is patient quality of life using a valid, established evaluation instrument. However, no such data are available. Most surgeons and, we are sure, most members of the public would be fairly confident in saying that other things being equal, life without a stoma would probably be of higher quality than life with a stoma. The need for a second operation to close the stoma would probably also reduce quality-of-life scores.

Another relevant outcome is comparative cost. Since primary closure leads to fewer complications, appears to have a considerably shorter length of hospital stay and eliminates the need for a second operation, it is probably less expensive from both health care and societal perspectives. Although a large and detailed cost-effectiveness analysis would be necessary to prove this, the results of the meta-analysis are sufficient to con-

clude that primary closure is probably the more cost-effective option.

Primary repair of penetrating colon injuries is as safe as fecal diversion, and has a lower rate of complications. This meta-analysis presents convincing evidence in favour of primary repair in all patients with such injuries. Nelson and Singer¹ have answered the question posed very convincingly. The evidence presented, and their discussion of evidence from other non-randomized studies, demonstrate that primary closure is the preferred treatment. This should now be the standard of care for patients with penetrating colon injuries, in all but the most unusual circumstances.

Competing interests: None declared.

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